REMINISCENCES OF LOS ALAMOS

1943—1945

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REMINISCENCES OF WARTIME LOS ALAMOS

To refresh my memory about Los Alamos I read a couple of books on the wartime period but got confused and irritated because they certainly didn't jibe with my recollections. Everything in books looks so simple, so easy, and everybody was friends with everybody. But even more annoying was that none of my young associates, who did most of the work, were even mentioned. So I got in touch with people who were there and who are now in the vicinity of Boston, and did some telephoning to my former associates in other cities. In a way, what follows is a group recollection, but I take full responsibility for it.

Before coming to Los Alamos I was probably the senior expert in the whole explosives division of the National Defense Research Committee (NDRC). I began research on explosives in June 1940 (when NDRC was organized) and by 1943 thought I knew something about them. I was also involved in a National Academy of Sciences committee late in 1941, which advised President Roosevelt that the atom bomb was feasible.

The man who deserves full credit for developing the concept of implosion, necessary to explode a plutonium weapon, is S. Neddermeyer. He and his assistant visited our NDRC Explosives Research Laboratory in Bruceton, near Pittsburgh, in the summer of 1943. We made the first implosion charges for them, fired them off, and the visitors went away rather pleased with themselves and with us. The reason that I went to Los Alamos was that James B. Conant, who was chairman of NDRC and knew my views on military high explosives (that they could be made into precision instruments, a view which was very different from that of military ordnance), was also the effective policy maker of the Manhattan District, as the co-chairman of the so-called military policy committee guiding General Groves. I began going to Los Alamos as a consultant in the Fall of 1943, and then pressure was put on me by Oppenheimer and General Groves and particularly Conant, which really mattered, to go there on full time. I didn't want to, partly because I didn't think the bomb would be ready in time and I was interested in helping to win the war. I also had what looked like an awfully interesting overseas assignment all fixed up for myself. Well, instead, unwillingly, I went to Los Alamos. That gave me a wonderful opportunity to act

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as a reluctant bride throughout the life of the project, which helped at times.

After going there on full time at the end of January 1944, I brought there from the explosives division of the NDRC quite a few people, who became the key operators in the implosion project: Linschitz, Koski, Kauzmann, Hornig, Jackson, Patapoff and others, later Roy. I found a very small operation under Neddermeyer -- only a few people working -- and a continuing angry conflict between Neddermeyer, who is now a professor at the University of Washington, and his boss, Captain Parsons, later Admiral Parsons, now unfortunately dead, who was the head of the Ordnance Division. I was put in the middle, between them by Oppenheimer, and that was a very uncomfortable place. So uncomfortable that two or three months later I wrote a memo to Oppenheimer, asking to be released from the project. That didn't work and instead I got a more authoritative job.

By the summer of 1944 I was supposedly the boss of all the implosion work, but, as I was still under Parsons, the conflicts continued. Theoretically, the official channel of communications was from Neddermeyer to me, to Parsons, to Oppenheimer. Well, it didn't work that way. Basically, Neddermeyer believed that the implosion research should be done by a small group, in a consecutive set of experiments until the right way of doing it was achieved. Now, I don't know whether you know what implosion is. We ended with a spherical charge of high explosives, almost five feet in diameter with a metallic pit in the middle. In the center of the pit was the plutonium fissionable material. Our job was to induce the pit and the plutonium to be compressed in an orderly fashion under the extreme pressure of a detonation wave, many millions of pounds per square inch, into something very much smaller than it normally was, whereupon it would become supercritical. A nuclear reaction would then spread and a big bang follow. Neddermeyer believed that this had to be discovered in a scientific, orderly fashion. Captain Parsons was a Navy ordnance officer, accustomed to developing mass products, and therefore felt very differently about how the work should be done. Soon after I arrived Captain Parsons brought to Los Alamos a Mr. Busby, an old Navy ordnance civilian to be in charge of explosives manufacture. And so the issue very soon became who, Busby or Kisty, [i.e., Kistiakowsky] knew more about explosives. Busby was a little difficult because when you disagreed with him about what was safe and what was unsafe, he would say "and have you ever picked up a man on a shovel?" So Mr. Busby designed and had built the first explosives casting plant. It was a monstrosity from our point of view. Actually, what he had built in the spring of 1944 was never used afterwards.

Then came the summer of 1944. I insisted that we build another plant

according to our concepts and proposed a completely new site for it, in Pajarito Canyon. Captain Parsons rejected that and insisted that the existing S-site be expanded and that's where the plant went. But it was built according to our designs, and it worked. One of the reasons I didn't like the S-site was that to get the raw explosives there they had to be trucked all the way over the mesa, right through the center of the Los Alamos project, with the whole theoretical division sitting in offices on one side of the road, Oppenheimer's office (and mine) on the other, and with hundreds of wild WAACs and GIs driving trucks and jeeps there. I can assure you that a truck loaded with five tons of high explosive (H.E.) going off there would have wiped out 90% of the brains in those temporary buildings. The roads leading from the main project to the S-site, which was a few miles south of there, were dangerously rough. Once, so an apocryphal story goes, when General Groves visited Los Alamos, I took him to the S-site in my jeep that had the springs made inoperative by wooden blocks inserted under them. General Groves was rather rotund in shape. As a result of that trip the roads over which H.E. were moved were improved.

We had developed in NDRC complete different notions from those of the Ordnance Corps about what was dangerous and what was safe in handling high explosives. At Los Alamos we handled them by the ton, whereas one gram of explosive going off in your hand will finish off the whole hand. By the end of the war we had cast and machined, following our rules of safety, tens of thousands of H.E. castings without a single accident. And we did it without barricades, which are required by ordnance rules. There was no time to build barricades. So we just worked. The S-site was managed by the Army Captain Jerry Ackerman, a civil engineer before the war, with a few very young Navy lieutenants and ensigns, like Hopper, Wilder, Chapell and a large number of GIs, who were called SEDs (Special Engineer Detachment), doing most of the work. Also there were quite a few civilians such as Price and Gurinsky, who were mostly in a development section, to find how to cast these explosive slurries. They were mixtures of explosives, a so-called Composition B and another explosive, Baratol, which we had developed at the Bruceton (Pittsburgh) NDRC Explosives Laboratory, and which was used for lenses. Up to 25 tons of H.E. was trucked up the hill monthly during the most active period. The manufacture of the H.E. charges was really hard, tough and dirty work. A lot of people thought it was also very dangerous and because of that I went to S-site very frequently and tried out new operations simply to show confidence in what we were preaching.

One of the very important contributors to our success was Master Sergeant

Tenney, in peacetime a doctor of physics, who developed non-destructive X-ray inspection of castings. Until the middle of 1944 all we could do was to cut a casting with a saw to see whether it was homogeneous or had bubbles and cracks. Gradually Jerry Tenney's project grew into a mammoth operation with million volt X-ray and gamma ray sources to inspect all the castings.

The implosion experimentation involved exploding the castings and trying to find out whether they did what we hoped they would do to the metal inside. It was originally carried on by Neddermeyer, Greisen, Lynch, Kauffman, and an increasing number of SED assistants. What became more and more clear was that if an explosive charge was detonated simultaneously from several points then trouble developed. Remember that detonation waves are like acoustic waves, more or less, but they develop enormous pressures and travel through the explosive at a speed of seven to eight thousand meters per second (five miles per second). At the point where two detonation waves meet, a metal core is squeezed into a high velocity jet and complete chaos develops. In other words, this sort of thing makes implosion impossible. To solve that trouble, first Linschitz, then Koski, then Jim Tuck, a member of the British technical mission who was sharing in our work, began experiments with explosive *lenses*. I won't go into details except to say that the principle on which these lenses is based is just like that of the optical lenses, where you have two media of different refractive index, such as air and glass, and therefore a different velocity of light in each. If you have two explosives with different detonation velocities, and you put them together in the right way you can shape the wave, and instead of having it expand, make it to converge. It's not simple, but it can be done. And I arranged with the Bruceton NDRC laboratory, particularly with Drs. MacDougall and Eyster, to develop an explosive, Baratol, for the slower component of the lens.

There is an interesting story about Baratol. Lord Cherwell, who was originally professor of physics Lindemann at Oxford, and was *the* science advisor to Winston Churchill, came to Los Alamos in 1944. He had been a very kind and helpful host when I spent part of the spring and summer of 1941 in England learning about explosives technology from the British. He was very considerate then and took me to his 'private' explosives research laboratories. His staff there was developing trick things for the British commandos. Very interesting ideas. Well, so he fancied himself as an explosives expert. I was trying to be extra polite, so I took him around Los Alamos and gave him absolutely the whole story. He listened to me and told me that Baratol was going to be no good for the lenses, that I should use commercial dynamite. I explained to him why, on theoretical grounds, dynamite couldn't work and we parted friends. A little later Oppenheimer called me in to tell me about Churchill's personal cable to Roosevelt saying that certain people, specifically Kistiakowsky at Los Alamos, are barking up the wrong tree, since Baratol is not going to work, and that they should use dynamite. I suggested to Oppenheimer that he could tell the sender to go to hell. Naturally it ended by my agreeing to set up a group to study dynamite lenses. So I went through a rather large X (Explosives) Division personnel list, singled out individuals who hadn't contributed anything, constituted a group out of them, and so dynamite didn't delay the project in the slightest.

Because of organizational difficulties and the growing importance of implosion, in July and August 1944 there was a big reorganization. Parsons continued to head the Ordnance Division but two new divisions were created, X and G. I ran the X, which did the high explosive charges development work and the experiments with the already developed observational methods on implosion dynamics, also lens development, and some other tasks. We ran this division in a very democratic fashion, with frequent meetings for discussing the results of what we had done, scheduling new experiments, scheduling and allocating the output from the S-site and so on. Unfortunately, my young associates like Linschitz, Koski, Kauzmann, Tenney, and others were regarded, high above, as too young to be made into group leaders because then they would be on a par with people like Edwin McMillan, Luis Alvarez, and Edward Teller. So they were made into section leaders, and Commander Norris Bradbury, professor of physics at Stanford, was brought in August from the Dahlgren Naval Proving Grounds to head group X-1, which was comprised of about five sections headed by these youngsters. They operated the sections in a pretty autonomous fashion because each had his own experimental site for safety reasons -- maybe as much as a mile apart. And the sites were fairly self-contained, perhaps even possessing a lathe, a few draftsmen and so on.

The G (Gadget Physics) Division, Robert Bacher being put in charge of it, undertook to develop new experimental methods of observing the movements of the pit after being struck by the detonation wave, and also to design the pit, including of course the plutonium sphere. I might add that this reorganization was partly due to the theoretical conclusion that implosion was the only way in which an effective plutonium bomb could be made. I welcomed this separation of functions because by then the total implosion staff became much too difficult to manage. But in a few months' time our relations with the G Division became not very happy because the G groups put tremendous demands on the S-site to provide them with tricky explosives castings, which

the S-site simply could not meet without stopping supplies for the X Division experiments and interfering with its own development of better casting methods. So we as a group began to feel that the G Division was merely delaying the progress of the implosion project by working on such complex experimental methods that they would never yield anything useful in time. I must admit that G Division people didn't share our views. Since Bob Bacher is not a shrinking violet, he and I had some very heated arguments. Oppenheimer, I think, backed him more than me – maybe Bob Bacher has a different view – and so Oppie even brought Charles Lauritsen from Cal Tech to Los Alamos to help the X Division as his, Oppenheimer's, personal assistant. That added to my troubles, because the key source of delay in providing more castings was that we couldn't get enough molds in which to make castings. These molds were really precision devices because the shapes into which explosives had to be cast were such that we could not machine rough shaped castings with the tools available to us at the S-site. They had to be cast into precise shapes. Some were not small. For instance, full sized castings weighed up to 100 pounds or so. Some X Division people, especially Bob Henderson and Earl Long, were of tremendous help in providing precision molds. Gradually huge machine shops were created at Los Alamos for this and other purposes – more than 500 machinists and toolmakers were working there at one time.

I think that certainly not the least factor in our success was Master Sergeant Fitzpatrick (SED), our X Division procurement and scrounging wizard. We learned early in the game that if we followed the rules of the Manhattan District, we wouldn't get anywhere. The slow facility construction and procurement delays, and the shortages of various materials were really a painful problem. When Charlie Lauritsen came to Los Alamos he was critical of our procurement efforts. So Charlie said that his Cal Tech Navy rocket project staff will solve our molds problem; but they didn't in time, and so the spring of 1945 was very short on molds and very rich in recriminations.

By late 1944 so much pessimism was developing about our ability to build satisfactory lenses that Captain Parsons began urging (and he was not alone in this) that we give up lenses completely and try somehow to patch up the non-lens type of implosion. So, in early 1945 we had a top-level meeting with General Groves present in which a kind of battle royal was fought, in a friendly way, between Parsons and me because I felt that we couldn't patch implosion without lenses and he felt we couldn't make the lenses. Oppenheimer in the end decided for the lenses and that was that.

In the early days of the implosion project, to get detonation started all over a sphere of explosives there was one electric detonator firing a branching

circuit of Primacord. Primacord is a rope-like detonating fuse which transmits the detonation wave at a speed of some three miles per second. The ends of this circuit were embedded at appropriate points in the explosive charge. The use of multiple electric detonators, or blasting caps, was objected to by Parsons' Ordnance Division because the Army-Navy rules require that there be a mechanical 'gate,' that is a piece of metal, between a detonator and the main explosive charge which has to be withdrawn before firing. For instance, in an artillary shell, the 'gate' is withdrawn by centrifugal force after the spinning shell leaves the muzzle of the gun. This makes the shell safer to handle before firing, because detonators are dangerously sensitive to impact and heat. To install as many such gates as would be necessary in the implosion device was just an engineering nightmare. Furthermore, the electric detonators then in existence had horribly poor timing, so that to explode them simultaneously looked, to us at least, absolutely impossible. Mind you, since detonation waves travel almost a centimeter per micro-second, the timing we were concerned about were fractions of a millionth of a second. We did a lot of experimenting with Primacord ordered in fancy, expensive, special batches. Lt. Shafer did most of this work. The Primacord just wasn't good enough.

Luis Alvarez in the summer of 1944 started experimenting with electric detonators and, I must say, completely to my surprise, found a way of setting them off in such a way that the simultaneity was very acceptable. Then new detonators were so designed by Alvarez, with my help, that we were able to persuade Captain Parsons to drop the requirement of 'mechanical gates.' So we abandoned Primacord. Then Bainbridge, Fussell, and Hornig in our division began work on what we called the X unit, an electric device to fire these new detonators simultaneously. Alvarez moved to other problems in the G Division, Bainbridge went to head the Alamogordo or Trinity test site, and Greisen in our division took over the detonators in the spring of 1945. That turned out to be a very nasty and unfinished problem because we couldn't get reproducibility with the Alvarez detonators. Most of them worked fine, but since one had to have many for each implosion, and even the failure of one could be a catastrophic failure, even doubling the circuit didn't give adequate assurance. So Kauzmann and Jackson did a very clever piece of physico-chemical research on the explosive charge in the detonators and found a way of making them reproducible.

To test or not to test the plutonium bomb was a very hot issue in the fall of 1944. Oppenheimer and I were pleading with General Groves that there had to be a test because the whole scheme was so uncertain. But General

Groves said he couldn't afford to lose all that plutonium if the chemical explosive went off but there were no nuclear explosion. General Groves was very sensitive about what would happen to him after the war and whenever he didn't like something, he'd say, "Think of me standing before a U. S. Senate committee after the war when it asks me: 'General Groves, why did you spend *this* million or *that* million of dollars?' " Well, it was difficult to answer that, too. So we proposed to test the bomb in a confining vessel, and a couple of very bright engineers in our division, Carlson and Henderson, designed Jumbo, a 200 ton ellipsoidal steel tank, with wall thickness nearly twelve inches, in the center of which, they believed, several tons of high explosive could be exploded and, if there were no nuclear reaction, everything would stay inside, although the vessel would stretch out quite a bit. And so plutonium could be recovered after everything cooled down. In the spring of 1945, Jumbo was delivered to the Trinity test site, but by then more plutonium was coming off the production line and we felt much more confident of implosion. So Jumbo was set up just half a mile away from the Trinity tower, and never used.

By the way, a sad story about Jumbo Groves was very sensitive about Jumbo and kept accusing me personally about it. Once he said to me, "Now you are responsible for Jumbo, and it is not being used, so what am I going to tell to the Senate? Look here, we got that cyclotron from Harvard for Los Alamos. How about you convincing Conant that when he goes back to the presidency of Harvard he should accept Jumbo in trade for the cyclotron at Los Alamos?" Actually Jumbo had a very sad end after the war. In the fall of 1945 Groves suddenly remembered that ghastly thing and told his aide-de-camp to arrange for a test, so at least he would be able to tell the Senate committee that the thing had been used. Well, you know the way it is in the Army. The orders filtered down, down from Washington to Albuquerque, Albuquerque to White Sands Proving Grounds, and so on. It ended with some poor lieutenant taking a box of high explosive and instead of suspending it in the center of Jumbo, putting it on the bottom of Jumbo and firing it. Naturally, the high explosive knocked out a nice clean hole in Jumbo, so that was the end of the matter. The U.S. Senate never caught on to the Jumbo extravaganza and its battered remains are still there, half buried near the Trinity site.

The Special Engineering Detachment (SED) was very important. General Groves insisted that, in distinction to technical staff civilians, most of whom lived quite comfortably in a highly hierarchically organized society, the enlisted personnel be given only what the Army regulations stipulated as the minimum comforts: minimum housing, minimum recreation, minimum food

facilities. And this meant 40 square feet per man in the barracks, including part of the recreation area. Try to recreate yourself in that area. So the poor SEDs, of whom we had more than a thousand at the end of the war, really felt themselves the pariahs of Los Alamos. And moreover the commanding officer of the SED detachment was a South Bostonian. You must have read a little about South Boston in the Fall of 1974 when the Boston schools opened. Well, he was a true South Bostonian and besides he had been wounded in the heel, in fact the back of the heel, on the first day of Eisenhower's African landing. As a result, he simply hated the world, and especially the longhaired scientists, notably those from Harvard. Since he was not told, as many other military weren't (nor the machinists, of course), what the purpose of Los Alamos was, he loudly described all of us as draft dodgers who were just escaping Army service and having fun here. He insisted that the SEDs be awakened by a reveille and be mustered daily and do calisthenics and keep the barracks in order and even wear caps and salute officers on the streets. These were insulting ideas to most of the SEDs. Since my division had the most SEDs and we had very good relations, there finally came to me an SED delegation who said they were going to complain to the War Department unless that officer was removed. I said, "look, this is mutiny in war time -- don't," but they said "then do something about it." So I naturally went to Oppenheimer and he, not for the first time, argued with General Groves -- to absolutely no avail. Later, when Groves came again to Los Alamos I asked permission to talk to him. He said, "yes, while I am being driven back to Albuquerque." So after midnight I got into his car and we went on that 2½ hour trip to Albuquerque and argued about SED. Of course Groves immediately told me that as a civilian I had no business to tell him anything about Army matters. And I said that the SEDs were part of my technical staff, they had to report to me, they had to work for me, and therefore I had the authority. Well, I got absolutely nowhere. I then used my ultimate weapon: I said I would resign. Still seemingly no effect. Well, I didn't have time to resign, because in a little while the South Bostonian became the Manager of the Officer's Mess and the SED got a new commanding officer, a very nice tall Texan, who made absolutely no disciplinary demands and spent most of his time drinking in the company of a very cute WAAC. Eventually both slid with their jeep down a canyon-side, but that was after the war. And they didn't get hurt, they were so relaxed.

The Trinity test was the climax of our work. The site is 200 miles south of Los Alamos in a part of a bombing range called Jornada del Muerto desert, not very near Alamogordo, but in that region. The G Division and Oppenheimer insisted that a perfect replica of the charge to be exploded at the

Trinity test be fired at Los Alamos using the so-called magnetic method observation technique. We were desperately short of adequate H.E. castings. A few days, maybe a week, before the test, after borrowing an electric dentist's drill, I spent most of a night drilling holes in unperfect castings where Sergeant Tenney had discovered bubbles of air. I reached the bubbles and then poured molten explosives into these channels to fill the bubbles and drill holes and in that way make the castings better, because we knew that when there were bubbles the detonation wave became distorted. Just like the bubbles in an optical lens, or scratches do. In that way we repaired enough castings so that the two assemblies, each weighing several tons, were assembled, with Norris Bradbury in charge of this particular job.

The assembly to be fired at Trinity had to be trucked through Santa Fe and Albuquerque and a lot of people outside the X Division thought that these assemblies were far more dangerous than ordinary iron aircraft bombs, which they really weren't at all. So I rather vividly remember that I got on a truck with a loaded assembly and drove it around the Los Alamos roads which were certainly worse than any roads we would encounter on the trip to the Alamogordo site, just to show that nothing would happen. A few minutes past midnight on Friday the 13th, my choice, because I believed in unorthodox luck, Bradbury and I took the assembly in a convoy to the Trinity site.

Bainbridge had been in charge of planning and then developing the Trinity test site since 1944. But in 1945, as this project expanded and became more and more tense, he was separated from the X Division and became the head of a new TR Division. This grew of coorse to a very large size, because so many experiments and observations were being readied for and at the Trinity site. Hornig, about July 10, 1945, (six days before actual test), took several X units to Trinity because various observational instruments had to be triggered by the firing of the X unit for simultaneity. And so they had to use the X unit to assure synchronization. A day or two later there came a violent storm and the X unit being used fired prematurely by itself. Well, a human storm followed over Hornig's head for incompetent design because people imagined this happening when it was connected to the bomb. Until, that is, Hornig discovered that the grounding wire was pulled off or busted or something and the X unit got a huge static charge from the lightning. That sort of relieved the human storm.

We arrived two days later, early on Friday, to encounter another emotional scene. The second X unit failed dismally the evening before and Don Hornig spent most of the night being grilled – and the word is grilled, not questioned – by Oppenheimer and Bacher, being accused of incompetent work and so on. As soon as we arrived I was told what they thought of me as a manager of these incompetent youngsters. So Don Hornig and I went finally to look at the X unit which was located under the bomb tower and discovered that the people who were using it while testing their instruments, horribly abused the unit. The unit was designed to be used only once, and it was tested about ten times to make sure that it worked fine. Well, these characters used it in rapid succession hundreda of times and overheated it so that some soldered joints melted. This discovery relaxed the atmosphere in the headquarters. But Saturday morning another awful thing happened. A telephone message came from Los Alamos that the G Division's magnetic method group found their H.E. assembly's detonation so bad that they could guarantee the failure of the bomb at Trinity. So of course I immediately became the chief villain and everybody lectured me – Oppenheimer, Groves, Bush, who was there by then with Conant. Did they tell me what was wrong with me! Only Conant was reasonable. Sunday morning another phone call came with wonderful news. Hans Bethe spent the whole night of Saturday analyzing the electromagnetic theory of this experiment and discovered that the instrumental design was such that even a perfect implosion could not have produced oscilloscope records different from what was observed. So I became again acceptable to local high society.

On Friday or Saturday, Bacher and his group inserted the plutonium into the pit, then Bradbury and a couple of SEDs replaced the H.E. castings, which had been taken out to be able to get into the pit. The bomb was hoisted to the top of the hundred foot tower. There Hornig installed a fresh X unit and Linschitz and Kalecka, an SED in our division, finally inserted the detonators into the charge. I had very little to do the last two days, just watch others. Sunday night I spent partly up on the bomb tower with Bainbridge and two others below me because weather was bad for the test which therefore had to be delayed and Groves insisted that there was danger of sabotage to the bomb – a perfectly idiotic idea. So we were supposed to watch it with a sub-machine gun in the hands of Captain Bush and that sort of thing. Finally, the decision to fire was made at five-thirty in the morning. We drove back, first unlocking the box containing the safety switch and all of us solemnly watching as Bainbridge closed the switch and locked the box. Then we drove a mile, repeated the operation on a switch box in a trench, and finally got to the locked box in the control bunker six miles away, opened it and closed that switch. The thing was ready to be fired. I had nothing to do and so just before the time counting came to zero I went up to the top of the control bunker, put on dark glasses and turned away from the tower. I didn't

think anything would happen to me, although I was sure that implosion would work, because I was rather convinced that the physicists exaggerated what would happen from a nuclear point of view. Well, I was as wrong as they were on Saturday. As soon as the explosion took place, Oppenheimer and others rushed to join me and I slapped Oppenheimer on the back and said, "Oppie, you owe me ten dollars" because in that desperate period when I was being accused as the world's worst villain, who would be forever damned by the physicists for failing the project, I said to Oppenheimer, "I bet you my whole month's salary against ten dollars that implosion will work." I still have that bill, with Oppenheimer's signature. Now, there is something more to that story, because after VJ day there was a lot of nonsense published in Sante Fe and other more or less local papers about the Trinity test, that I was, as they put it, a temperamental Russian who lost his self-control and embraced and kissed Oppenheimer, whereas all I did was slap him on the back and say, "Oppie, you owe me ten bucks." So, after VJ day, when we had a sort of post-mortem, and all the group leaders of the project must have been there, Oppie said, "you must have read these stories; I want to testify that they are wrong, George never kissed me, but I am now going to kiss him." He did that and gave me the ten dollar bill.

Let me now make a few remarks of a more personal nature. General Groves was really a terror to his subordinates. He was a skillful manager, but he always did things so as to make life difficult for the subordinates. However, he and I had perfectly good relations. I think he saw me as more manly than the effete physicists because of my explosives work. I was to him a sort of kindred spirit.

I had very good relations also with Oppenheimer. He had an incredible ability to have all the threads of this enormous project in his mind and to make the right technical decisions. As to his managerial skills, that's a little different story. Conant regularly came to the project because he was the really active member of the Military Policy Committee, and he and I usually had private technical conversations. I met several times with Niels Bohr when he was at Los Alamos but only because he wanted to know from me how implosion was progressing. I naturally dealt a lot with the British group – Chadwick, Peierls, Tuck, Penney. The man who eventually turned out to be a spy, Fuchs, managed to put himself into a very important position. He was a member of the Theoretical Physics Division, like Professor Hirschfelder, but he also arranged to be appointed as the liaison between Theoretical Physics and X Division, so that he sat in on all our discussions and planning meetings.

At Los Alamos I was treated like a VIP. I had special housing, a tiny little

stone cabin that used to be a diesel station before the war. Oppenheimer sold me, for a nominal sum, one of his saddle horses, a beautiful Quarter horse named Crisis. The Army maintained a horse stable in Los Alamos and for a small fee took care of a half-dozen or so private horses. We never worked on Sundays, that was a hard and fast rule, so on Sundays I rode horseback in the mountains. In the winter of 1944–1945 we built a ski slope nearby using explosives to cut down the trees. We had a lot of surplus plastic explosive, the demolition explosive, and if one builds a half necklace around the tree, then the explosion cuts it as if you had a chain saw – and it's faster. A little noisier, though. Then we scrounged equipment to build a rope tow and it became a nice little ski slope. From my friends in Washington, I got a couple of skimobiles which weren't like the modern skimobiles but more like jeeps on tracks. They weren't very good but we managed to use them to go skiing further out. I played a lot of poker with important people like Johnny Von Neumann, Stan Ulam, etc. You see, before coming to Los Alamos, I went through a very rigorous and expensive poker training school in Washington, headed by Roger Adams, NDRC member and my boss. So when I came to Los Alamos I discovered that these people didn't know how to play poker and offered to teach them. At the end of the evening they got annoyed occasionally when we added up the chips. I used to point out that if they had tried to learn violin playing, it would cost them even more per hour. Unfortunately, before the end of the war, these great theoretical minds caught onto poker and the evening's accounts became less attractive from my point of view.

We did quite a lot of partying on Saturday nights, but there was *one big* party after VJ day in Bacher's house, and by the time we had quite a few refreshments my physicist friends started telling me that I must arrange for a 21-gun salute. Of course I didn't have the guns. Finally, I got into my jeep and got one of my younger friends out of bed, who then insisted on driving the jeep. We went to the H.E. magazines, got out 21 boxes of Composition B (50 pounds each), laid them out on the field (I was stumbling a little because the field was very rough) and fired them off. It was a very impressive performance but when I got back to the party those bastards told me I fired 22 shots.

DISCUSSION

Question: What field did Von Neumann work in in those days?
Kistiakowsky: Von Neumann throughout the war was a consultant to Los Alamos. He spent, toward the end, I think, as much as half of his time there.

When he was there he was a member of the Theoretical Physics Division. But even earlier, you see, he did the important job of convincing Oppenheimer and others who questioned Neddermeyer that if the implosion were perfect, the forces acting on the pit would be such as to compress the plutonium to a density that one would presumably find near the center of a star or something like that, and thereby very rapidly change it from a sub-critical, in the nuclear sense, to a highly super-critical assembly. Von Neumann was very much in favor of implosion. In 1941–1942 he did some important theoretical work for the explosives division which I headed in NDRC (Division 8), so his interest in explosives was genuine.

Question: Some of the information passed by the Rosenbergs to the Russians concerned the lens and your aspect of the project. How critical was that information?
Kistiakowsky: That is something I have been asked about before. From reading Mr. Khrushchev's memoirs and some other information, I gather that the Russians had started the atom bomb project quite early in the war. For instance, there is the story that when Stalin met with Truman in July of 1945 and Truman told him of a successful Trinity test the day before, Stalin acted as if it were of no interest to him at all. According to Khrushchev, however, he immediately sent a message to Moscow to put Beria in charge of their project. So the project must have been going. Under those conditions the very crude sketches of Greenglass could not have been of great importance. Maybe of no importance whatever. I think what probably was far more important is what Fuchs transmitted because he was able to send detailed results of calculations and also the information on problems of timing the detonators and a lot of other things like that. I think though that even this did not make a tremendous difference, because it turned out that to cause the implosion is a much easier job than we thought it was. But it might have accelerated the Russian bomb by a year or two. This would be my guess. But it's only a guess.

Question: I have often wondered how much sooner World War II would have been over if there had been no atom bomb project and so many of our scientists and resources had not been shifted from conventional arms to the atom bomb. Surely this would have speeded up the German part of the war and probably the Japanese.
Kistiakowsky: Well, it's a speculation in which I will not engage. After all, the Manhattan District, which was active during the war, was not a very large project – two billion dollars in three years – at a time when the total war

effort was costing nearly a hundred billion dollars a year. So when you refer to resources, you mean the very special resources that were in tremendous demand, but whether they would have altered the war's progress, I don't know. Whether Japan would have delayed surrendering is also an unsolved question. I think that what one now knows suggests that they would have surrendered anyway. We were told, and I am not sure whether the military intelligence people came to Los Alamos or whether Oppenheimer brought the news from Washington, that according to our Naval Intelligence, (as you know there are several kinds of intelligence: there is human intelligence, there's animal intelligence, there's the military intelligence), that according to Naval Intelligence Japan was ready to continue fighting the war and that we would have to go through with our plan of invading the main Japanese islands in November 1945 and that there would probably be a million casualties before the war ended. That information had a tremendous impact on me, on my thinking about the military use of the bomb. Since then, it's become pretty clear that Japan was really far closer politically to surrender than that estimate indicated.

Question: Do you think that it is easy for a small group of terrorists to construct nuclear weapons?
Kistiakowsky: That is an estimate which I've heard advanced by Dr. Theodore Taylor. I don't think so. Perhaps I am too proud of our work. I think you couldn't build a reliable bomb without doing some experimentation with explosives and that experimentation is more than a small private group could undertake. On the other hand, it would not require the resources of a Soviet Union or India to build a bomb. It could be done on very much of a shoestring. The bomb wouldn't perform as well, but there is quite a leeway. Certainly anything approaching the complexity of the Trinity bomb and of the implosion bomb dropped over Japan, I would say, would require a very major effort. Assuming you already have the fissionable material, I would estimate several million dollars worth of effort and a couple years. One could build a much cruder bomb, but even then it would require some resources. Let's say a country possessing one technical university and some military forces and some kind of proving grounds could do it.

Question: What does a lens have to do with implosion? Is it a lens like the lens in my glasses?
Kistiakowsky: It's not an optical lens, though it functions similarly. The explosive lens bends the explosion wave going through the explosive. To an

innocent it looks like another piece of explosive. It's made out of two different explusives, and that's the secret of it.

Question: Were the scientists at all disturbed that Truman decided to drop the bomb on a population center instead of testing the bomb in an unpopulated area? Did the scientists have anything to say about how the bomb would be used?
Kistiakowsky: Well, the scientists in the Chicago part of the Manhattan District – the Met Lab, which by the spring of 1945 was almost inactive because it had done its job – became extremely active trying to stop the military use of the bomb over a city, urging a harmless demonstration instead. At Los Alamos we had some conversations on the subject and I must admit that my own position was that the atom bomb is no worse than the fire raids which our B-29s were doing daily in Japan, and anything to end the war quickly was the thing to do. Other people felt differently but there was no organized movement at Los Alamos to stop the bomb use. I changed my mind afterwards but I was very much influenced by the military intelligence estimate of what would happen that summer. I won't go into detail of some of the more technical arguments against making a demonstration, but it looked unfeasible to me.

Question: Was the second bomb that was dropped on Nagasaki necessary? Didn't just a very minor point in the surrender terms remain, namely the safety of the Emperor?
Kistiakowsky: The Japanese position was that they could not surrender unconditionally because that would probably mean that the Emperor would be dethroned and maybe even executed. They would be willing to surrender if the monarchy would be retained. I think our excuse for dropping the second bomb was very weak. As I recall it, the argument was: let's drop them quickly – one, two – to give the Japanese government the idea that we have an unlimited supply. We only had two. It would have been at least a month, or maybe two, before another bomb could be produced and dropped over Japan. But, of course, we at Los Alamos had absolutely no control over their use once the bombs were shipped overseas. This happened within ten days or so after Trinity, as soon as we manufactured the new set of H.E. castings. The other type of bomb, the gun type, which was used over Hiroshima, was shipped overseas even before the Trinity test. After that, there was nothing we could change.

Question: You said that you had to cut through red tape, to cut corners in order to make any progress. I wonder if you would describe some of those methods.

Kistiakowsky: No sir, they will remain secret. Well, let me tell you this much. There was an extraordinarily elaborate procurement system for military security purposes. We had to order everything from an office in Los Angeles. That office, acting as a part of the University of California, and not of Los Alamos, which was secret, then ordered things from all over the country. These things were shipped to Los Angeles and then were delivered from there to Los Alamos. That resulted in terrible delays and errors. We found ways of getting around them.

Question: Was there any awareness in your group of German progress along the same lines?
Kistiakowsky: The rumour of German progress was the thing that was egging us on. But by the end of 1944 it became pretty obvious that the Germans didn't have the bomb and wouldn't have it in time. Then the argument presented to us became that we must end the war with Japan as quickly as possible. But in earlier days it was a very real fear that the German bomb would be built and would win the war for them. As a matter of fact, that interesting job assignment abroad I missed because of going to Los Alamos had to do with trying to find out what the Nazis were doing on the A-bomb.

Question: Was there just one piece of plutonium in the bomb?
Kistiakowsky: We had plutonium cast into two hemispheres that fitted very neatly together. And we had no more plutonium at Los Alamos. The plutonium, however, was being manufactured very rapidly at Hanford and so within a relatively short time after Trinity a second sphere was made at Los Alamos and taken to Tinian Island.

Question: What effect did the ending of the war with Germany have on Los Alamos?
Kistiakowsky: We celebrated it.

Question: I mean in terms of your feelings about the project, its motivation, things like that.
Kistiakowsky: Well, naturally it became less important, the whole thing became less intense, as we began to realize that we were certain to win the war. But, as I said, the feeling was conveyed to us that Japan was very far from surrender, that the war would continue for a long time. That continued to provide, I think, the emotional cohesion of the laboratory staff.

SCIENCE

V-3?

The terrible novelty of V-2 had by no means worn off yet, but London last week was already abuzz with speculation about V-3—supposedly an atomic bomb. Allied bombers renewed their attentions to Rjukan, Norway, the site of a heavy-water plant which the Nazis have recently rebuilt after its destruction by the R.A.F. and Norwegian patriots last year. Meanwhile, British censors passed a London dispatch giving the most circumstantial account to date of atomic bomb possibilities.

According to this account, the Nazis may have discovered an entirely new approach to atomic explosives. Before wartime censorship blacked out all talk of atomic experiments, it was known that most scientists put their atom-smashing hopes mainly in cyclotronic bombardment of atoms with deuterons—the heavy hydrogen nuclei derived from heavy water. Individual atoms have been smashed, but in a bomb atoms must explode in quantity, each disintegrating atom setting off others. The new Nazi experiments are said to be along lines suggested by the composition of the "White Dwarf," companion of Sirius, which is the densest known star.

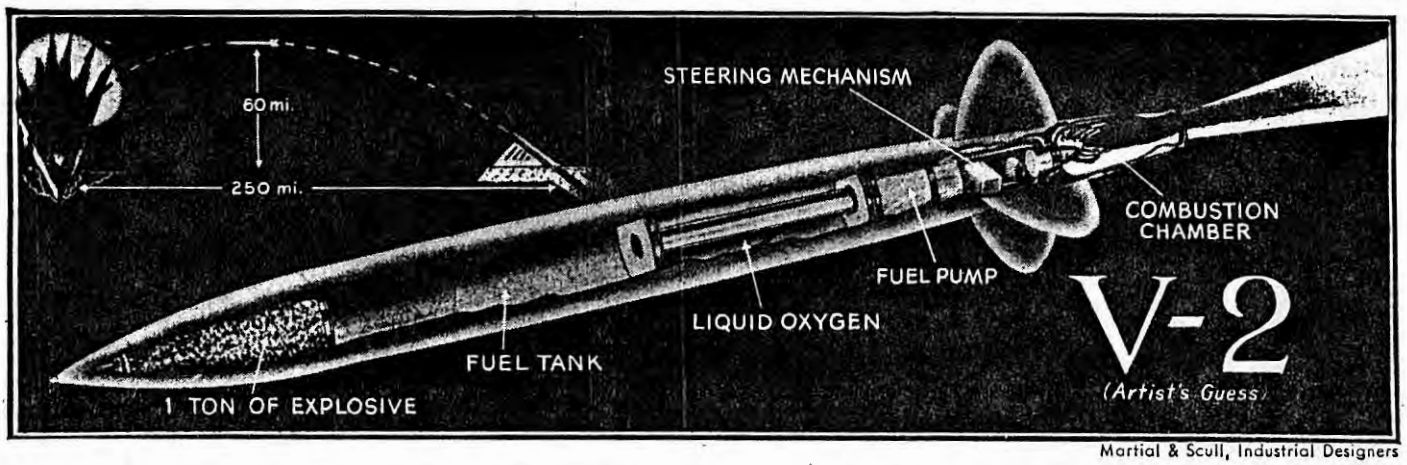
The White Dwarf is so dense (specific gravity: 61,000) that a cubic inch of its substance weighs about one ton. Physicists believe that ordinary atoms could not be compressed to such density, and they suppose that the tremendous pressures and high temperature of the White Dwarf have broken up its atoms, letting their space-hungry electrons escape and leaving only the much more compact atomic nuclei.

The speculative London report suggested that the Nazis are using the same pressure principle to crush atoms. The crusher: A "Neuman" demolition charge, which explodes inward instead of outward. Used in a sphere, the Neuman charge might develop pressures of tens of thousands of tons per square inch at the center, perhaps enough to disintegrate an unstable atom such as uranium and release its explosive atomic energy. British scientists believe that such an explosion, though not far-reaching in area, would develop unheard-of violence at the point of impact.

What Is V-2? Last week V-2 was still almost as great a mystery as V-3. If the British had recovered any duds for examination, they were keeping mum about it. Some Hollanders claimed they had seen V-2 launched from bare ground; others, from 80-ft. concrete pits. Some experts thought it could have been launched from barges off the Dutch coast. V-2 was variously reported to be guided by radio, by gyro compass, by fins, by spinning. But on one thing experts agreed: V-2 is a self-contained rocket, carrying its own oxygen and traveling at such speed (1,000 to 3,500 m.p.h.) that ordinary antiaircraft defenses are useless against it.

The consensus: V-2 is probably propelled by alcohol or gasoline and liquid oxygen. It has a warhead with about a ton of explosive, a supply of compressed gas (perhaps nitrogen) to force the fuel into the combustion chamber, and fins to keep it on a set course. It is believed to carry at least seven times the weight of its explosive in fuel. It probably has a series of jets, operated in succession to keep the rocket going on its long course (and perhaps helpful also in steering). One plausible reconstruction, by Martial & Scull, Manhattan industrial designers, indicated a steering mechanism in the tail (*see cut*). It seems unlikely that V-2 is steered by radio, since V-1 is not and, at the heights to which V-2 climbs (60 miles or more), accurate observation to correct its deviations from the set course would be difficult. With a trajectory like that of a long-range shell, dropping sharply from its peak height, V-2 is probably launched at about a 30° angle from the ground for its 250-300 mile flight.

Glimpses of the Moon. Last week Britain's famed jack-of-all-sciences, J. B. S. Haldane, philosophically predicted a big postwar future for V-2, which he thought could rise to 200 miles if fired vertically. Mused Haldane: "It could take photographs . . . [of] the sun and perhaps other heavenly bodies. . . . For the cost of a day of war, it should be practicable to send a series of rockets round the moon and photograph its far side."



Martial & Scull, Industrial Designers

Whither, Swift?

The chimney swift is one of the swiftest small birds alive (up to 150 m.p.h.). Until last week no one had ever been able to say where it was going in such a hurry, when it took off for the winter. The U.S. Fish and Wildlife Service had banded 375,000 swifts, never traced a one to its winter hideout. For lack of a better theory, some naturalists surmised that the swifts buried themselves in swamps to hibernate.

Indian hunters in a South American jungle produced the first evidence of the swifts' winter whereabouts—13 bands taken from birds they had killed. The place: the Yanayaco River valley in Peru. The birds had been banded in Tennessee, Illinois, Connecticut, Alabama, Georgia, Ontario. Cried Frederick C. Lincoln, official U.S. observer of bird migrations: "One of the most important ornithological discoveries in at least two decades."

Ornithologist Lincoln added that the swift discovery left only one North American bird-mapping mystery unsolved: where nests the bristle-thighed curlew?* No one has ever found its nest or eggs.

Superman of the Waldorf

The late Nikola Tesla was a spectacular, eccentric scientist and showman. Sure that his name will outlive Thomas Edison's, Tesla's admirers hold that he and Michael Faraday were the greatest electrical discoverers of modern times. Last week one admirer, who according to the inventor himself understood him "better than any man alive," published the first Tesla biography—*Prodigal Genius* (Ives Washburn; $3.75). The author: John J. O'Neill, science editor of the New York *Herald Tribune*.

O'Neill, throwing off journalistic reserve, describes Tesla as "a superman—unquestionably one of the world's greatest geniuses." O'Neill credits him not only with inventing the polyphase alternating current generator and Tesla induction motor, which

* Not to be confused with the bird that the late great Poet Yeats rebuked:

O, curlew, cry no more in the air
Or only to the waters in the West,
Because your crying brings to my mind
Passion-dimmed eyes and long heavy hair
That was shaken out over my breast.
There is enough evil in the crying of the wind.